False premises lead to false conclusions: an examination of Prince & Valencia and the MSC Pre-Assessment.

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Both the Prince & Valencia essay and the MSC Pre-Assessment (Daume et al.) contain erroneous conclusions based on false premises. I will focus on areas in which I have professional expertise (evolutionary biology), and on points that are too obvious to be ignored. I will spell out the problems in very clear language, so that there is no room for ambiguity, because the consequences of false management decisions may undo 15 years of recovery efforts for abalone at San Miguel Island, as well as the remainder of southern California. The critique points overlap significantly with those identified independently by Cynthia Button (Comments on the arguments presented by Prince & Valencia 2009: November 2009), also submitted to DFG Commission as supporting documentation.

Main conclusions:

1: the Allee effect is live and well, and should be part of fisheries management discussions and decision making.
2: Wykoff ledge data are interesting anecdotal data points that should be treated with much caution, because they represent statistical outliers. Most other NPS sites indicated ***declining*** abalone populations.
3: whichever is correct (Jiao vs. Wykoff), there are strong reasons for not starting a fishery until the discrepancies have been resolved.
4: if MVP is disliked, do provide a solid, proven alternative.
5: the critique of uncolonized but suitable habitat confounding population estimates is misconceived.
6: good scientific analysis presents also the undesirable results, or alternative interpretations. This crucial element is missing in both documents critiqued here.
7: poaching as a pure negative consequence of opening a fishery must be fully addressed in TAC models.
8: assumed "safe" fishing level has lead to stock collapse in the past; statistical error propagation problems and type 2 error need to be fully addressed.
9: Prince & Valencia's Bootstrap is not a predictive method, hence, can not be advanced as an alternative to Jiao's model. It cannot demonstrate sustainability.
10: the SMI population is an ESU and merits special considerations to ensure long-term survival.
11: El Niño mortalities must be fully and explicitly considered in models and fisheries proposals. A proposed fishery must demonstrate long-term sustainability before fishing starts.
12: the MSC color-coded scoring glosses over fundamental uncertainties and sever contraindications to a fishery.

1) The Prince & Valencia essay attacks the Allee effect concept as an evolutionary *non-sequitur*. For one, they argue abalone "want to reproduce", a teleological fallacy.
Second, they ignore the white abalone (H. *sorenseni*) and the black abalone (*H. cracherodii*), both of which also "want to reproduce", which have not shown any significant recovery. In fact, both are still listed as Federally Endangered Species. How come that these two are not showing any stock increases? Allee effect! That the effect comes in degrees should go without saying (tech speak: density-dependent factor with non-linear response function), and *H. rufescens* fortunately was not been decimated to such an extent that it, too, is now listed as an endangered species.

Furthermore, they have no explanation for why red abalone at other sites in southern California have not recovered. The abalone's desire to reproduce must be just as strong as those on San Miguel Island. The National Park Service (NPS) data (Figure 1) clearly confirm the Allee effect; note the strongly fluctuating population densities and the overall declining trend in the majority of sites.

Third, they ignore the fact of extinction. Geiger & Groves (1999) wrote a review on fossil abalone, noting several clear cases of extinction (e.g., *H. barbadensis*, some of the New Zealand fossil species). How do species go extinct? Through reduction in population, until they are so small that no more reproduction is going on, or in other words, the Allee effect.

Conclusion 1: the Allee effect is live and well, and should be part of fisheries management discussions and decision making.

2) The Wykoff ledge data is used both by Prince & Valencia and by Daume et al. as a counterfactual to Jiao's decreasing model prediction for the SMI abalone population. Three things are neglected here. 1) The Wykoff ledge data is a limited sampling of narrow spatial data series, while DFG/Jiao's is based on large scale, wide geographic area sampling. Hence, the Wykoff ledge data should be considered a statistical outlier and omitted from further consideration. 2) It is clearly not indicative of either large-scale patterns at SMI, and certainly not in the remainder of Southern California, where abalone densities still are negligible. 3) The other NPS sites generally show declining abalone population trends (Figure 1). The use of Wykoff ledge data *only* by Prince & Valencia is a clear case of cherry-picking available data to lend false credence to a predetermined outcome. Such actively misleading practices must be condemned in the strongest possible terms, and strike at the heart of scientific fact finding.

[The Bren-school student modeling has not been reviewed by AAG or external experts.]

Conclusion 2: Wykoff ledge data are interesting anecdotal data points that should be treated with much caution, because they represent statistical outliers. Most other NPS sites indicated *declining* abalone populations.

3) The conclusions reached from the contradicting Jiao model and the Wykoff ledge data are viewed with a bias towards the fishery-friendly alternative; one could also say that they are viewed through rose-colored glasses. The mere fact that those two are contradictory should raise alarms. Either Wykoff ledge data are statistically anomalous (as demonstrated above) and Jiao's model is correct (best view, population change so small that it disappears in the noise of the data), then the population is in no shape to be fished, experimentally or otherwise. Or Wykoff ledge data are correct for SMI and Jiao's model is fundamentally flawed. In this case, it demonstrates that we lack the most elementary understanding of how the SMI population behaves. Starting a fishery when we lack basic knowledge of how the population behaves is unjustifiable.

Conclusion 3: whichever is correct (Jiao vs. Wykoff), there are strong reasons for not starting a fishery until the discrepancies have been resolved.

4) Both (Prince & Valencia, Daume et al.) critique the MVP concept. But what is the alternative? None is presented by either. The advantage of MVP is that sampling procedures are rigorously defined, and implementation is relatively easy.

What is needed are solid, proven counterproposals, most likely based on nearest neighbor (NN) distances. These need to be specified to the same degree of detail as line transect/MVP. An incomplete list includes:

- definition of metric (e.g., mode of NN distance, or high 95% confidence interval of estimate).
- critical value of metric (when to stop fishing, the MVP equivalent of NN).
- assessment of statistical power (should be better than +/-30% of line transect).

- selection of starting points (random, if not how, why: how to adjust metric? Problems of how to deal with selectively removing zeros in statistical analysis arise, which is very close to data manipulation in the bad sense of the term).
- radius of search area (how does it affect metric?).

Conclusion 4: if MVP is disliked, do provide a solid, proven alternative.

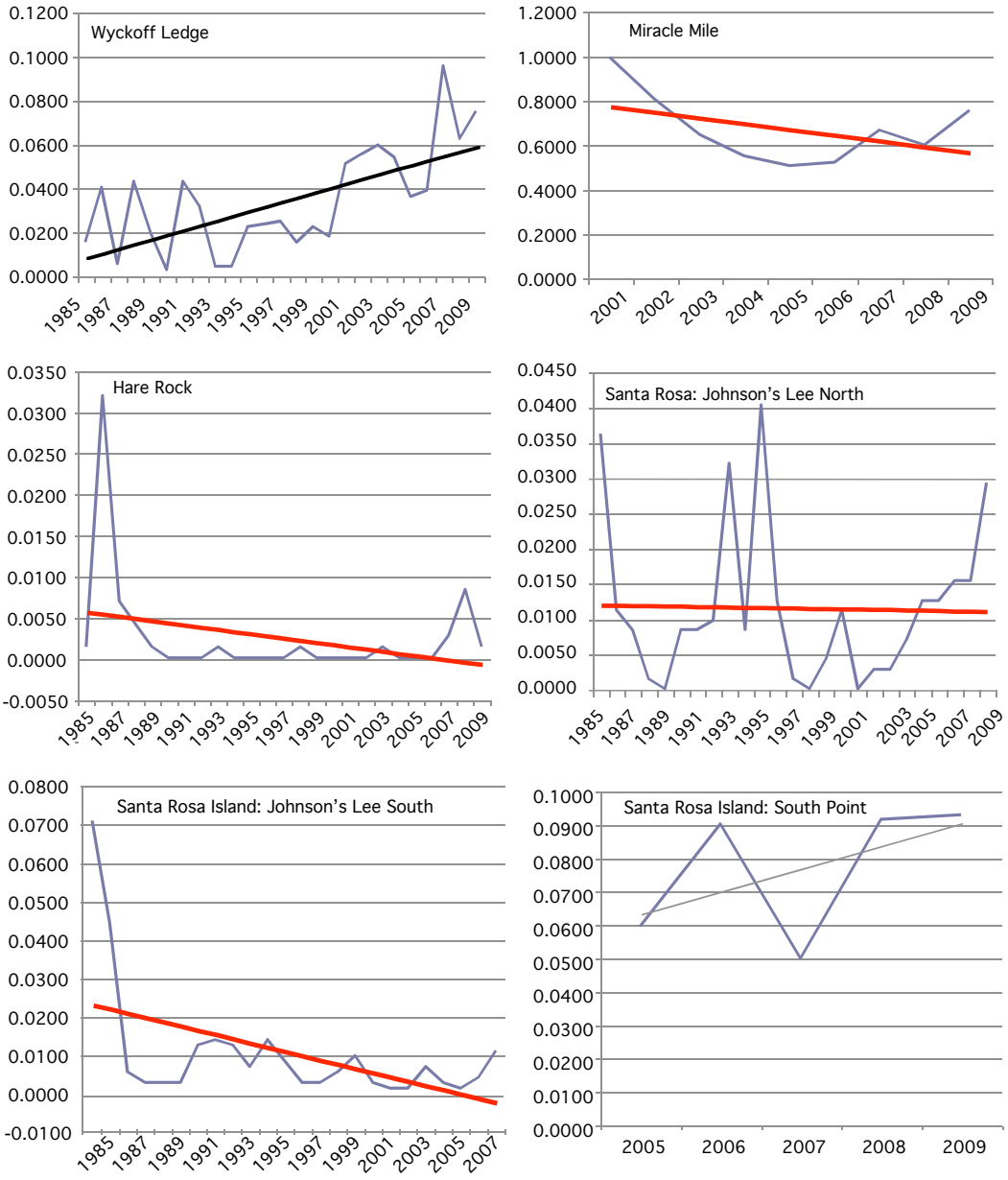

Figure 1. Comparison of all NPS sites showing overall erratic population trends, confirming the deleterious impact of the Allee effect. The majority of sites show ***decreasing*** abalone populations on San Miguel and also Santa Rosa Islands. Wykoff ledge represents and exception to the overall pattern and cannot be used to justify the start of a fishery or to critique Jiao's model predictions. Source of data and raw graphs: NPS.

5) Not all suitable habitat is occupied by abalone. This argument is taken by both to indicate that the extrapolation of MVP based on suitable habitat is mistaken, because abalone do not occupy all those areas considered suitable by humans. It ignores the fact of slow recolonization of habitat after a massive stock reduction.
Consider a desert. Reforestation will first start at the edges, then slowly creep inwards, possibly with some islands of new growth somewhere in the middle. There is not suddenly grass/oaks growing all over the Sahara.
The same is most likely the case with abalone at SMI. After decades of overfishing, certain areas are now inhabited by other organisms. Homing spots are covered by sponges, barnacles, tube worms, and seaweeds. For this former abalone habitat to be recolonized will take decades. Hence, the zeros in the line transect data are historically seen accurate. While we do not have good data from subtidal areas, the photos in Cox (1962) of black abalone carpeting the intertidal speak volumes. Abalone *did* occur at *very* high densities, covering every last square inch of suitable habitat.
Conclusion 5: the critique of uncolonized but suitable habitat confounding population estimates is misconceived.

6) Prince & Valencia try to show by selective data elimination (low density survey data), that abalone occur at high densities at SMI, taken at face value by Daume et al. One could apply the same methods and selectively and sequentially eliminate all high estimates and come to the conclusion that there are hardly any abalone at SMI. Which view is correct? Possibly both. On the one hand, there are a few high density islands, which are good for sources of new recruits. On the other hand, the low overall density shows that the overall area has a long way to go to reach full recovery.
Conclusion 6: good scientific analysis presents also the undesirable results, or alternative interpretations. This crucial element is missing in both documents critiqued here.

7) Daume et al. completely overlook the problem of poaching; searching the document for "poach" to catch spelling alternatives resulted in zero (0) hits! It cannot have escaped MSC with three professional abalone biologists, that a high value fisheries species, and specifically in this case abalone, is a poacher's magnet. All AAG participants are in agreement that poaching is likely to increase with any opening of a fishery.
Where are the safeguards in the TAC to account for poaching? The same also applies to accidental killings (falling rocks, gouging) and stress (repeat measurements, handling stress) of the specimens in the area. The CAA code of conduct is a nice-looking touch, but the proposals are unenforceable, hence, have no real consequence.
Here is a new proposal: subtract the 10-fold amount of confiscated poached abalone (assuming a 10% chance of being caught: no reply from DFG when asked for estimates) from current or future TAC. This will also encourage all legal participants to protect the resource to a greater degree. It may, however, encourage vigilante behavior as has been witnessed in the Mexican fishery.
Poaching is of a magnitude that it cannot be ignored in modeling. E.g., Blank & Gavin (2009) estimated that 15% of fishermen exceed annual limits, up to 72% violate daily limits, and 23% take undersize abalone in the northern California recreational abalone fishery.
Poaching is a gift that keeps giving. If fishing is closed after one year, poaching will likely continue at a higher level than pre-fishery. Once abalone fishing enters the public mind, it will be difficult to re-establish a no-fishing rule. It is a well-known psychological fact, that exploitation of natural resources is perceived as an unregulated public right (Dayton, 1995).
Conclusion 7: poaching as a pure negative consequence of opening a fishery must be fully addressed in TAC models.

8) The use of age-class abundance, with assumed natural mortality and assumed safe fishing level is based on multiple false premises. Hence, I treat them in turn.
Assumed mortality is a guess. This is not something that should be used in scientific assessments. There are appropriate scientific methods such as mark-recapture studies. Daume et al. label their assumed 15% mortality rate as "conservative", while Prince & Valencia assume 10% mortality. A 50% difference in assumed mortality is highly significant and introduces high levels of error into any model. If TAC is taken as a fraction of mortality, then the higher the mortality (15% vs. 10%), the *less* conservative the TAC (*contra* Daume et al.).
Assumed safe fishing level have the same problems as assumed mortality. Additionally, this is exactly what was used in the old days, and hence, is the "method" that resulted in the fishery's collapse. Have we really learned nothing from past mistakes? It very much seems so. Proper analysis requires knowing intrinsic growth rate r, which can be obtained from net population growth minus mortality as determined by mark-recapture experiments. Or recruitment success could be determined with settlement plate assays and invasive surveys, the latter harbor its own problems due to its invasive nature.
The compound use of assumptions leads to the statistical problem on error propagation, a very serious issue. Hence, if anyone wishes to pursue this avenue, then the appropriate statistical procedures should be applied. None of that has been done by either Prince & Valencia or Daume et al.
Additionally, TAC estimates have focused on avoiding under-exploitation (addressing statistical Type 1 error). However, doing it at the expense of accidentally overexploiting the resource leads to fisheries collapse as demonstrated with abalone as well as many other fisheries around the globe. The second problem is called Type 2 error in statistics and should be fully explored (see Dayton et al. 1995, Dayton, 1998). Given the detection level limit of +/- 30% of population size, overexploitation can only be recognized after >30% of the population has been destroyed.
Conclusion 8: Assumed "safe" fishing level has lead to stock collapse in the past; statistical error propagation problems and type 2 error need to be fully addressed.

9) Bootstrap analysis of Prince & Valencia is presented as an alternative to Jiao's model, an untenable position. Bootstrap produces confidence intervals for the number of individuals in a given stock assessment (2008 DFG cruise data). It is not predictive. It can not be used to indicate what will happen the next year. Jiao's Model is critiqued for not making accurate enough predictions, while the Prince & Valencia Bootstrap is *non*-predictive! Jiao's Model is critiqued for having too large confidence intervals, while Prince & Valencia's Bootstrap has none what so ever, or range between zero and infinity!
Conclusion 9: Prince & Valencia's Bootstrap is not a predictive method, hence, can not be advanced as an alternative to Jiao's model. It cannot demonstrate sustainability.

10) The special status of SMI abalone as the only reasonably recover***ing*** (not recovered) stock has also been ignored. The population genetics work has shown, that SMI has only limited connectivity to other areas (DFG forensic lab presentation to AAG). The good news is, that it is not a short term significant source of stock for other areas (but in the long term over evolutionary time, it may well be significant, considering the importance of rare dispersal events; see any treatment of dispersal biogeography and Braje et al. 2009). On the other hand, it also means that SMI is an Evolutionarily Significant Unit (ESU). Hence, standards to ensure survival of this stock are higher, because it is an ESU.
Conclusion 10: The SMI population is an ESU and merits special considerations to ensure long-term survival.

11) El Niño mortality has been ignored by Daume et al. The experimental data obtained from SMI abalone for the AAG process at Bodega Marine Labs indicate expected El Niño mortalities

of 30–50% above background mortality. The experimental data apparently were not provided by CAA to MSC (why not?), but the *Rickettsia*-like prokaryote causing withering foot syndrome (WFS) is mentioned (p. 19). The temperature dependence of WFS severity should be common knowledge to anybody who has ever worked with abalone (Daume, Mundy, Mayfield), hence it is a mystery why this has not been further discussed.

WFS may prove to be a catch 22 problem. High abalone density is desirable to increase the probability of WFS-resistant abalone strains appearing, but high densities also seem to increase pathogen transmittance in the population, hence, may cause higher mortality during El Niño years prior to resistant strains emerging and establishing itself through a selective sweep [standard evolutionary theory]. Low abalone densities will lower the probability of resistance to arise, but will lower transmission rate. On the other hand, stock reduction due to El Niño of low density abalone population will cause proportionally greater reduction in recruitment potential due to Allee effect. Modeling this multifactor problem, and demonstrating no-harm sustainability under those conditions will be exceedingly difficult.

Conclusion 11: El Niño mortalities must be fully and explicitly considered in models and fisheries proposals. A proposed fishery must demonstrate long-term sustainability before fishing starts.

12) Daume et al.'s green/yellow coding of the various aspects of a potential fishery strains credibility. Stock status yellow, despite the fact that the stock is well below MVP, prior history of stock collapse, and threat of El Niño/WFS. Stock rebuilding yellow, despite marginal (~6%) change in DFG surveys, declining population model, and expected 30–50% El Niño mortality due to WFS. Harvest control yellow, despite zero consideration of poaching. Assessment of stock yellow, despite only 30% change being detectable. Incentive for sustainable fishing green, despite no consideration of poaching, and no model indicating sustainability. Decision making process green, despite no hard cut-off values being specified.

Conclusion 12: the MSC color-coded scoring glosses over fundamental uncertainties and severe contraindications to a fishery.

Upshot

The two documents do *not* represent scientific un-biased accounts, but are pro-fishery-biased, overlooking and suppressing serious problems. Given the past demise of abalone fisheries in California, in conjunction with novel issues such as El Niño and WFS, a hyper-precautionary approach is indicated. Don't hope for the best, but consider the worst case scenario a given (Type 2 error consideration). If under the worst case scenario a fishery can be justified with high degree of confidence, then try a small experiment, but no sooner. At present we have insufficient survey data and model output to do anything but watch from the side lines.

From an AAG-historical perspective, it is interesting to note the early concerns about the "race to fish" and how to avoid it. It is incomprehensible how anyone could want to start even an experimental fishery given the serious problems, uncertainties, and deficiencies outlined here. Furthermore, the aggressive timetable pushed by CAA to get a decision from DFG Commission cannot be described as anything but a "race to fish." Prudent stewardship of a resource looks different.

Literature cited

Blank, S. G. & M. C. Gavin. 2009. The randomized response technique as a tool for estimating non-compliance rates in fisheries: a case study of illegal red abalone (*Haliotis rufescens*) fishing in Northern California. Environmental Conservation 36: 112-119.

Braje, T. J., Erlandson, J. M., Rick, T. C., Dayton, P. K. & Hatch, M. B. A. 2009. Fishing from past to present: continuity and resilience of red abalone fisheries on the Channel Islands,

California. Ecological Applications, 19: 906–919.
Cox, K. W. 1962. California abalones. Family Haliotidae. California Department of Fish and Game, Fisheries Bulletin 118: 1-133.
Dayton, P. K., Thrush, S. F., Agardy, M. T. & Hofman, R. J. 1995. Viewpoint. Environmental effects of marine fishing. Aquatic Conservation: Marine and Freshwater Ecosystems 5: 205–232.
Dayton, P. K. 1998. Reversal of the burden of proof in fisheries management. Science 279(5352): 821–822.
Geiger, D. L. & L. T. Groves. 1999. Review of fossil abalone (Gastropoda: Vetigastropoda: Haliotidae) with comparison to Recent species. Journal of Paleontology 73: 872-885.

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